Bortkiewicz' Alleged Discovery: the Law of Small Numbers

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Abstract

Ladislaus von Bortkiewicz (1868 – 1931) published his law of small numbers (LSN) in 1898. The name of that law was unfortunate; moreover, lacking any mathematical expression, it was only a principle. Many commentators described it, but my paper is the first ever attempt to examine it thoroughly, and I argue that Kolmogorov's unsubstantiated denial of its worth is correct. For a few decades the law had been held in great respect and thus deserves to be studied.

Key words: Law of small numbers; Poisson distribution; stability of statistical series; theory of dispersion

1. Introduction

I begin with a short description of statistics in the second half of the 19th century and the beginning of the 20th century and introduce my main heroes; in conclusion, I describe here the preparation of Bortkiewicz' booklet on the LSN, quote his definitions of that law and discuss its name. Debates around the LSN took place in the early 20th century, and it is opportune to mention that by that time the Continental direction of statistics became established, and that Bortkiewicz believed that his law strengthened the Lexian theory, or, in other words, essentially contributed to that direction. Actually, however, he was gravely mistaken; the LSN, never expressed in a quantitative, mathematical way, was deservedly forgotten, but it certainly turned general attention both to the Poisson distribution and to the Lexian theory.

1.1. Statistics in the Second Half of the 19th Century

The most eminent statistician of that period until his death in 1874 was Quetelet (Sheynin 1986; 2001a, § 3). His field of work was population and moral statistics; he did not try to apply the statistical method in biology. In that latter direction he could have preceded the British biometricians, but his religious feelings prevented him from studying Darwin whom he never mentioned.

Quetelet had introduced elements of probability theory into his moral statistics (inclinations to marriage and crime), and after his death German statisticians, without understanding that a statistical indicator did not apply to any given individual, rejected his approach as well as his alleged denial of free will. The same happened with Quetelet's belief in stability of crime under invariable social conditions (his reservation, not formulated clearly enough). However, a correct understanding of the dialectic of randomness and necessity together with the Poisson form of the law of large numbers would have dispelled that conclusion (if formulated in terms of mean values).

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Coupled with the general refusal to accept any probabilistic pattern excepting (not at all universally) Bernoulli trials, the situation became deplorable (Sheynin 2001a, § 5.3). The problem of testing the invariability of statistical indicators (naturally extended to cover those concerning vital statistics) became topical. Here is how Chuprov's former student (see §1.5) described the situation:

> Our (younger) generation of statisticians is hardly able to imagine that mire in which the statistical theory had gotten into after the collapse of the Queteletian system, or the way out of it which only Lexis and Bortkiewicz have managed to discover[1].

1.2. Emile Dormoy. The first to advance along the new road was the French actuary Dormoy (1874; 1878), but even French statisticians had not at the time noticed his theory, his discoverer happened to be Lexis (Chuprov 1959, p. 236). To specify: they barely participated in the development of the Continental direction of statistics (Keynes 1973, p. 431). Later Chuprov argued that the Lexian theory of dispersion should be called after Dormoy and Lexis (Chuprov 1926, p. 198, in Swedish; 1960, p. 228, in Russian; 2004, p. 78); however, Lexis achieved much more, and in addition it was his work that had been furthered by Bortkiewicz and Chuprov.

Bortkiewicz described the work of Dormoy and ranked him far below Lexis (Bortkiewicz 1930). In particular, he strongly opposed Dormoy who had decided that man, at least in large numbers, was subject to the "laws of fatality" (Bortkiewicz 1930, p. 44). I do not agree with him, nor do I understand how can the Lexian theory or his general views deny Dormoy's conclusion[2].

1.3. Wilhelm Lexis (1837 – 1914). He studied law, mathematics and natural sciences, but eventually turned over to social sciences and economics. He taught at several universities and became actively engaged in editorial work. From 1875 onward Lexis seriously contributed to population statistics, attempting to base it on stochastic considerations and thus advanced to the first rank of theoretical statisticians (Lexis 1903).

Bortkiewicz published a long review of Lexis (1903) intended for non-mathematical readers and described the latter's investigation of the stability of the sex ratio at birth, his statistical achievements in general, and his theory of stability of statistical series (Bortkiewicz 1904a). Much later he devoted two more papers to Lexis (Bortkiewicz 1915a; 1915b). The first one was the text of his oration on the occasion of Lexis' jubilee; Lexis, however, died soon afterwards. The second paper, which appeared in the *Bulletin* of the International Statistical Institute, was an obituary, and there, strangely, the author had in essence said nothing about Lexis the statistician. Still, in the first case, disregarding biometricians, he credited his teacher with a "new founding of a theory of statistics"(Bortkiewicz 1915a, p. 119).

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[1] "Unsere (jungere) Generation der Statistiker kann sich kaum jener Sumpf vorstellen, in welchen die statistische Theorie nach dem Zusammenbruch des Queteletschen Systems hineingeraten war und der Ausweg aus welchem damals nur bei Lexis und Bortkiewicz gefunden werden konnte." (Anderson 1963, p. 531).

[2] The only source describing Dormoy's life and work, which I was able to establish, was mentioned by Chuprov (1959, p. 236): A. Paolini, an article in the *Archivio di Statistica* for 1878, and it proved unavailable. Chuprov had not given the title of Paolini's article.

Finally, Bortkiewicz stated that Lexis' most important merit was not the introduction of *Q*, of his measure of stability of a statistical series, but the discovery that [assuming independent trials] it was never less than unity and depended on the extension of the "field of observation" (Bortkiewicz 1930, p. 40). I choose to say that his most important innovation was the introduction of a more general random variable into statistics.

1.4. Ladislaus von Bortkiewicz (1868 – 1931)

He was born into a distinguished Polish family in Petersburg and graduated there as a lawyer but became interested in statistics and economics and achieved worldwide recognition in both these fields. Since 1890 Bortkiewicz published serious work on population statistics, worked under the direction of Lexis in Göttingen and defended there his doctoral thesis. His German was perfect; it probably had been spoken at home and been the main language in his *gymnasium*. Most of his publications are in that language.

In 1901, on Lexis' recommendation he was appointed Professor at Berlin University, and there, in Berlin, he lived all his remaining life becoming ordinary professor in 1920. His style was ponderous, his readership tiny, partly because German statisticians (and economists) had then been opposed to mathematics. Many authors deservedly praised Bortkiewicz for his scientific work. Thus, he was called *The statistical Pope* (Woytinsky 1961, pp. 452 – 453), and Schumacher explained Bortkiewicz' attitude towards science by a quotation from the Bible (Exodus 20:3): *You shall have no other gods before me* (Schumacher 1931, p. 573)[3].

Bortkiewicz (and Chuprov) furthered the Lexian theory by determining the expectation and variance of its measure of stability, *Q*, a problem Lexis himself had not even hinted at, and Chuprov had also essentially specified (and greatly restricted the usefulness of) the conclusions of the theory.

The spelling of his name changed from Bortkevich (in Russian) to Bortkiewicz (in German).

1.5. Aleksandr Aleksandrovich Chuprov (1874 – 1926)[4]

Born in provincial Russia as a son of an eminent "non-mathematical" statistician, he became a mathematician with an eye to applying it to social sciences. He taught statistics in Petersburg and became Professor after defending his second thesis in 1908; the first one he defended in Germany in 1902. Under the influence of Markov with whom he corresponded for several years, Chuprov really turned to his initial goal although even much earlier he expressed himself as a partisan of Lexis and Bortkiewicz (Chuprov 1905). True, there also he wrongly stated that Bortkiewicz had rigorously justified the LSN (Chuprov 1905, p. 467).

Emigrating in 1917, Chuprov finally settled in Leipzig (Germany) as an independent researcher and died after a long illness in Geneva having lived there for a short while as a guest of an old friend.

Chuprov prepared many gifted statisticians. One of them was Oskar Anderson, a Russian German who emigrated in 1920 and became the leading statistician first in Bulgaria, then in (West) Germany. For about 30

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[3] For his biography see Gumbel (1968) and my own paper based on archival sources (Sheynin 2001b). An almost complete bibliography of his works is in Bortkevich & Chuprov (2005). Much information about Bortkiewicz, also based on archival sources, is in my book Sheynin (2006).
[4] See Sheynin (1990/1996).

years Chuprov corresponded with Bortkiewicz. I published their extant letters in their original Russian (Bortkevich & Chuprov 2005).

1.6. The Two Branches of Statistics

Lexis became the founder of what became called the Continental direction of statistics, whose forerunners were Bienaymé and even Poisson (Heyde & Seneta 1977, p. 49). In England, the periodical *Biometrika* appeared in 1902 with a subtitle *Journal for the Statistical Study of Biological problems*. Its first editors were Weldon (who died in 1906), Pearson and Davenport "in consultation with Galton". Pearson became the head of the *Biometric school*.

For a long time the two branches of statistics had been developing almost independently; moreover, the contributions published in *Biometrika*, for all their importance, were being dismissed on the Continent since they were usually of an empirical nature lacking stochastic support, see Sheynin (1996, pp. 120 – 122). In particular, I have quoted there Chuprov and Kolmogorov (who described the traits of the *Biometric* school):

Not "Lexis against Pearson" but "Pearson cleansed by Lexis and Lexis enriched by Pearson" should be the slogan of those, who are not satisfied by the soulless empiricism of the post-Queteletian statistics and strive for constructing its rational theory[5]

Notions of the logical structure of the theory of probability, which underlies all the methods of mathematical statistics, remained at the level of eighteenth century (Kolmogorov 2002, p. 68).

Some essential findings of the Continental direction had been independently discovered in England; thus, there exists a connection between the application of Q^2 and the chi-square method and analysis of variance (Bauer 1955). And it is opportune to mention Chuprov, whose important results only recently became sufficiently known (Seneta 1987).

The two last-mentioned commentators had not, however, aimed at a comprehensive study of the merging of the two branches of statistics into a single entity, but, anyway, the LSN had not helped in that process. For that matter, Bortkiewicz, contrary to Chuprov, had not recognized any merits of the Biometric school (Bortkiewicz 1915c).

2. Stability of Statistical Series (Lexis)

In his main contribution on statistical series, Lexis considered various types of statistical series (Lexis 1879). For my purpose, it is sufficient to mention series whose terms corresponded to a variable probability of the occurrence of the event studied. In other words, he abandoned the assumption of a random variable with a constant binomial distribution, – abandoned Bernoulli trials.

Suppose (my notation here almost coincides with Bortkiewicz' of 1898) that the observed proportions of successes in σ sets of trials, the result of each trial being based on n observations, are

[5] "Nicht 'Lexis gegen Pearson', sondern 'Pearson durch Lexis geläutert, Lexis durch Pearson bereichert' sollte gegenwärtig die Parole derer lauten, die, von der geistlosen Empirie der nachqueteletischen Statistik unbefriedigt, sich nach einer rationellen Theorie der Statistik sehnen". (Chuprov 1918 – 1919, 1919, pp. 132 – 133).

$$p'_1,\ p'_2, \ldots, p'_\sigma$$

corresponding to the *true* probabilities p_i. Their variance can be estimated indirectly:

$$\varepsilon_1{}^2 = \frac{\overline{p}'\overline{q}'}{n}, \overline{q}' = 1 - \overline{p}'$$

where $\overline{p}'$ is the mean of $p'_1,\ p'_2, \ldots, p'_\sigma$, whereas the direct estimate of the variance is

$$\varepsilon_2{}^2 = \frac{\sum_{i=1}^{\sigma}(p'_i - \overline{p}')^2}{\sigma - 1}.$$

Now, Lexis introduced a measure of the stability of a series, the *coefficient of dispersion*,

$$Q = \varepsilon_2/\varepsilon_1,$$

perhaps choosing the letter Q in honour of Quetelet. He called stability supernormal, normal or subnormal for $Q < 1$, $Q = 1$ and $Q > 1$ correspondingly. In the third case, as Lexis stated, the probabilities p_i underlying the different terms of the series were different; in the first case, the terms had to be somehow interdependent, whereas Bernoulli trials (independence of terms and constant probability p_i of the event studied) had only taken place if $Q = 1$. Bortkiewicz, however, noted (without supplying a reference) that Lexis had not discovered any supernormally stable statistical series (Bortkiewicz (1904a, p. 240), and Lexis had indeed restricted his attention to subnormal stability (Lexis 1879, § 10).

His conclusion about the three possible values of Q, based on common sense, seemed correct, but, mostly as a result of Chuprov's later and quite forgotten work, hardly anything was left from his theory (Chuprov 1918 – 1919; 1922b; 1926). Nevertheless, Lexis became the founder of what became called the Continental direction of statistics, – the study of population statistics by means of stochastic patterns, – whose forerunners were Bienaymé and even Poisson (Heyde & Seneta 1977, p. 49).

But how, in Lexis' opinion, did the probability vary? No universal answer was of course possible; nevertheless, he could have been more definite on that point. As it occurred, he thought that the variations followed a normal law (Lexis 1876, pp. 220 – 221 and 238), but then he admitted less restrictive conditions (evenness of the appropriate density function, – which is a later term) and noted that it was senseless to introduce more specific demands (Lexis 1877, § 23). Finally, he discussed "irregular waves" of variability (Lexis 1879, § 23). Bortkiewicz had not commented on this point. At the same time, Lexis made a common mistake by believing that the relation between the mean square error and the probable error remained constant (and equal to its value for the normal law) irrespective of the relevant distribution.

Concerning his first-mentioned pattern of variability, Lexis could have possibly attempted to apply somehow Newcomb's introduction of a mixture of normal distributions with randomly appearing different variances and zero parameters of location as an adequate law of error for long series of astronomical observations (Newcomb 1886; Sheynin 2002, p. 149). True, his suggestion was hardly practical since it demanded additional calculations and a subjective choice of the variances, of the number of terms in the mixture and of the probabilities with which each of these laws occurred, but at least it was possible for Lexis to heuristically support his research by that innovation. Apparently, however, neither he, nor Bortkiewicz had known about it.

3. The Law of Small Numbers (Bortkiewicz 1898)

3.1. Its Appearance, Definition and Name

Bortkiewicz had been preparing his publication for at least two years[6]. During that period Chuprov the mathematician helped him with his mathematics and advised Bortkiewicz to refer to Poisson[7].

Bortkiewicz twice defined the LSN:

> It turned out that the fluctuations found in the investigated series almost entirely corresponded to the predictions of the theory, which is precisely what constitutes the law of small numbers[8].

> … we may well call the fact, that small numbers of events (out of a very large numbers of observations) are subject to, or tend toward a definite norm of fluctuation, the law of small numbers[9].

These definitions describe a principle rather than a law.

Many authors, beginning with Chuprov and Markov, objected to the name itself, *Law of small numbers*. Chuprov called it "tempting but deceptive" (Bortkevich & Chuprov Letter No. 2 dated 1896) and Markov "once more demanded" its change (Bortkewich & Chuprov 2005, Letter No. 27 dated 1897). Much later, after Bortkiewicz' death, authors of several obituaries suggested another name, *Law of rare events*, e. g. Gumbel (1931, p. 232), whereas Mises earlier recommended a more suitable but hardly practical term, *Law of large numbers for the case of small expectation* [of the studied event] (Mises 1964, p. 108n). He had not repeated this remark in his obituary published in a rare source (Mises 1932).

In the same letter of 1897 (above), Bortkiewicz indicated that his attempt to publish his booklet in Russian by the Petersburg Academy of Sciences had failed owing to its expected appearance in German. There also, he described his talk with Markov. I quoted him and I only repeat now that Markov

> Considered the mathematical calculations [apparently, in a preliminary version of the booklet] correct, but did not dare pronounce his opinion

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[6] See the first letters in (Bortkevich & Chuprov 2005).
[7] Letter No. 2 dated 1896, ibid.
[8] "Es ergab sich, dass die bei den untersuchten Reihen gefundenen Schwankungen den Voraussagungen der Theorie fast vollständig entsprechen, worin eben das Gesetz der kleinen Zahlen besteht." (Bortkiewicz 1898, pp. VI).
[9] "die Tatsache, dass kleine Ereigniszahlen (bei sehr großen Beobachtungszahlen) einer bestimmten Norm der Schwankungen unterworfen sind bezw. nach einer solchen tendieren, das Gesetz der kleinen Zahlen wohl benannt werden". (Bortkiewicz 1898, p. 36).

concerning the work's scientific value since he believed that it belonged to statistics. (Sheynin 1996, p. 42).

3.2. Bortkiewicz (1898): Its General Contents.

The booklet contained an Introduction, three chapters and three appendices. In Chapter 1 he introduced the Poisson limit theorem and explained related material applied in Chapter 3. Chapter 2 was devoted to checking the agreement of the Poisson formula with statistical returns in cases of rare events (suicides and fatal accidents, including the study of deadly horse-kicks, so beloved by commentators). Modern authors confirmed that the agreement was "remarkably good" (Quine & Seneta 1987, p. 173). I examine Chapter 3 separately.

In Appendix 1 Bortkiewicz derived the first few moments of the binomial distribution in his own way using only a few of them. In Letter No. 7 dated 1896 (Bortkevich & Chuprov 2005), he explained to Chuprov that "now" he consented "to Markov's demand, without, however, resorting to generating functions and successive differentiation". The rejected (and now standard) method was likely comparatively new; anyway, Bortkiewicz could have well applied it in addition to his own, the more so since he liberally used power series and integrals in his Chapter 1.

He had been avoiding advanced mathematical tools. Much later he stated that the rejected method "was similar to solving the equation $2x - 3 = 5$ by determinants" [which was quite impossible!] (Bortkiewicz 1917, p. III). Concerning economics, Schumpeter argued that that attitude prevented Bortkiewicz from rivalling such scholars as Edgeworth (Schumpeter 1932, p. 339).

In Appendix 2 Bortkiewicz discussed *solidary trials*, but only in later contributions did he name his predecessors, Bienaymé and Cournot[10], and neither had he mentioned his own paper (1894 – 1896). Such, as he explained, were trials, or events, connecting several people at once (one of his examples: a group of travellers)[11]. Chuprov and, later, another author, without mentioning Bortkiewicz, indicated the other version of solidarity, – the negative correlation of trials, see Chuprov (1959, p. 234) and Geiringer (1942, p. 58).

Bortkiewicz explained the new case by drawing each time several balls at once from randomly selected urns with differing content. He derived a formula which somehow showed that solidarity led to $Q > 1$[12]. Much later Bortkiewicz applied the case of solidary trials to counter Markov's criticisms. (Bortkiewicz 1923, pp. 17 – 18). It would have been better to discuss solidarity in the main text rather than in an appendix.

Appendix 3 is Bortkiewicz' table of the Poisson distribution with four significant digits. Soper discovered there rounding-off errors whereas its

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[10] See Heyde & Seneta (1977, § 3.1) and Cournot (1843, § 117).

[11] Solidary action had been known in the treatment of observations as systematic errors (much later term) even to Ptolemy. Gauss thought that two functions with partly common observed arguments were not independent, and Kapteyn, in 1912, without mentioning him, even introduced the appropriate (but unnoticed) correlation coefficient (Sheynin 1984, pp. 187 – 189).

Another development in the same field concerning systematic errors was heuristically similar to applying the coefficient of dispersion (Helmert 1872, p. 274). The mean square error of measurement in triangulation can be computed during *station adjustment*, and after computing all the *conditional equations* corresponding to the chain. Such errors were present if the second estimate was larger.

[12] A modern derivation is due to Geiringer (1942).

author not really properly blamed his sister for this shortcoming[13], see Soper (1914) and B&C, Letter No. 138 dated 1914. The Poisson distribution had been noticed previously. Cournot recommended to apply it in actuarial calculations and Newcomb, in 1860, actually applied it for determining the probability that stars, uniformly scattered over the sky, can be situated near to each other (Cournot 1843, § 182; Sheynin 1984, pp. 163 – 164). Nevertheless, it was Bortkiewicz who made the Poisson distribution generally known.

3.3. Bortkiewicz (1898, Chapter 3)

Some formulas of § 13 of this chapter as well as some other expressions in subsequent sections contain *n*, the constant number of trials but he did not tell the reader that it meant the number of trials applied to calculate any term of the statistical series studied. Bortkiewicz had indeed said so, but only later, and only two commentators noted this point[14].

Bortkiewicz' main formula (unnumbered, on p. 31) of Chapter 3 is

$$Q = \sqrt{1+(n-1)c^2}$$

where *c* is a constant and Bortkiewicz naturally noted that *Q* decreased with *n*.

Several remarks are needed. First, the case of $Q < 1$, which was included in the Lexian theory[15], is here impossible since his *Q* differed from the Lexian coefficient, see below.

Later Bortkiewicz indirectly explained that in 1898 his main aim was to isolate the possible changes in the probability underlying a (number of) series (Bortkiewicz 1923, p. 15). Yes, he had isolated the influence of these changes (Quine & Seneta 1987), but, as it follows, had to abandon the case $Q < 1$. Second, and more important, it occurred that *Q* described not the desired magnitude, but rather the changes in *n*. Chuprov noticed this fact but only referred to Lexis (Chuprov 1959, p. 277). Bortkiewicz himself (1904a, p. 239) later stated that it did not at all follow

> That we ought to keep to small numbers and prepare our statistical data accordingly. On the contrary, for the most part it is of greater statistical interest to ascertain the physical component of fluctuations which, with moderate numbers, remain blurred[16].

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[13] The unmarried Helene von Bortkiewicz. In 1935 she visited Aline Walras, the daughter of the late economist Léon Walras with whom Ladislaus von Bortkiewicz had been in correspondence (published by Jaffé in 1965). In one of her letters of 1935 to Jaffé Aline described that visit. Helene had been subjected to the "horrors" of the Russian revolution, but then [in 1918] with "great difficulties" managed to join her brother in Berlin (Potier & Walker 2004, p. 88). The Germans, as Aline continues, suffer "de la misère"; Helene herself is drawing a small pension and is "prudent when speaking about Hitler". "He is not as malicious as is thought, and there will be no war. He should not be considered an ogre! He is a lamb!"

I can only add that Ladislaus was a member of the German Democratic Party, but had not been at all interested in internal policy, see Tönnies (1932/1998, p. 319) and Schumacher (1931, p. 576).

[14] See Bortkiewicz (1904b, p. 833), Newbold (1927, p. p. 492) and Bauer (1955, his formula (1)).

[15] Bortkiewicz remarked that the case should not be overlooked, that he arrived here at some "rather interesting results" and promised to acquaint Chuprov with them (Bortkevich & Chuprov 2005, Letter No. 135 dated 1914). I am unable to say anything else.

That component makes it possible to decide whether the underlying probability mentioned had changed.

Third, Bortkiewicz also introduced the Lexian coefficient denoting it Q' and stated, on p. 35, that it was approximately equal to Q. Later he noted that $EQ' = Q$ (Bortkiewicz 1904b, p. 833). Actually, as was readily seen from his formulas, an equality of that type held only separately for the appropriate numerators and denominators. Now, Q' was a fraction, and it was again readily seen that its numerator and denominator were mutually dependent. In such cases, as follows from a remark by Chuprov, the equality above does not necessarily hold (Chuprov 1916, p. 1791/2004, p. 40).

Bortkiewicz only admitted that the equality was not "fully rigourous" (Bortkiewicz 1918, p. 125n). This was an understatement: Chuprov subsequently devoted a paper to calculating the expectation of a ratio of two mutually dependent variables, and referred to Pearson's appropriate approximate formula (Chuprov 1922a), see Pearson (1897; 1910).

To repeat: 1) Bortkiewicz had only explained the meaning of n in a later contribution and, anyway, the coefficient Q did not describe the behaviour of the magnitude under study. 2) He had to abandon the case $Q < 1$. 3) Contrary to his statement, his coefficient Q' differed from the Lexian Q. Chapter 3 was not therefore satisfactory.

4. Discussions about the LSN

Chuprov listed four possible interpretations of the LSN, but the main point was the difference between its being the application of the Poisson theorem or a strengthening of the Lexian theory (Chuprov 1959, pp. 284 – 285). Then, in a letter to Markov of ca. 1916, Chuprov wrote that Bortkiewicz had been avoiding any discussion of the subject, and, in particular, did not comment on his (Chuprov's) statement above (Sheynin 1996, p. 68).

More is contained in Bortkiewicz' Letters NNo. 93, 101 and 106 dated 1909 – 1911 (Bortkevich & Chuprov 2005). In the first of these, he only stated that the LSN ought to be understood as "the agreement between formula and reality". In the second one Bortkiewicz emphasized that his views had not changed since 1898 and that he really had in mind a small number of occurrences of the studied event rather than its low probability. He also remarked that "Strange as it is, we find it ever more difficult to agree about the general significance and understanding of the l. of sm. numbers". And, in the third letter: "It is wrong to infer that I understand the low value of p [probability] as decisive". Did this mean that he was prepared to abandon the Poisson theorem (and the first two chapters of his booklet)? Anyway, he stated that his law

> Appears after all as the outcome of an extension of those Lexian investigations, and, in relation to theory, perhaps represents their conclusion[17].

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[16] "Dass man sich an die kleineren Zahlen halten und dementsprechend sich das statistische Material zurechtlegen soll. Es wird im Gegenteil meist eine größere materiell-statistisches Interesse haben, die physische Schwankungskomponente, die bei mäßigen Zahlen verschleiert bleibt, festzustellen." (Bortkiewicz 1904a, p. 239).

[17] "Erscheint nun als Ergebnis einer Weiterführung jener Lexis'schen Untersuchungen und bildet in theoretischer Beziehung vielleicht gar einen Abschluss derselben." (Bortkiewicz 1898, § 18, p. 38).

Much later Bortkiewicz forcefully confirmed that his LSN was closely connected with the Lexian theory, – and unjustly denied the negative binomial distribution (Bortkiewicz 1915c, p. 256).

Markov was the first to criticize the LSN, at first privately, then publicly stating that a large *Q* was hardly possible when small numbers were involved[18]. Bortkiewicz himself later expressed the same idea but did not attach any importance to it (Bortkiewicz 1923, p. 17). Then, Bortkiewicz, even earlier than 1916, refused to agree that *Q* ought to be *shelved* (Bortkewich & Chuprov 2005, Letter No. 135 dated 1914).The context did not imply the denial of the LSM; I cannot explain Chuprov's suggestion, but this disagreement is rather interesting[19].

5. Conclusion

Many other authors had later expressed their opinions, directly or tacitly, about the LSN. Romanovsky, who later became a leading statistician and head of the statistical school in Tashkent approvingly mentioned by Kolmogorov, called the LSN "the main statistical law" (Romanovsky 1924, vol. 17, p. 15). Among other authors who praised the LSN I name Gumbel (1931; 1968) and Mises (1932).

This support was not, however, unanimous. Czuber several times mentioned Bortkiewicz's booklet but did not say anything about it (Czuber 1921). Anderson not quite resolutely questioned the practical importance of the law, and, much later, Bauer, who stated that his research had appeared owing to Anderson's wish, did not mention it at all, see Anderson (1961, p. 531) and Bauer (1955). Neither did Mises although he described the Lexian theory (Mises 1928; 1972). In 1932 (see above), being an author of an obituary, he possibly was too generous.

It was Kolmogorov who became the first to state bluntly that the LSN was just a name given by statisticians to the Poisson limit theorem, but he did not elaborate (Kolmogorov 1954). My own verdict is that the LSN had indeed turned attention both to the Poisson theorem, and to the Lexian theory, but proved to be hardly useful otherwise.

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References

Abbreviation: JNÖS = *Jahrbücher für Nationalökonomie und Statistik*

[18] See Ondar (1977, Letters NNo. 71 and 84 to Chuprov dated 1916) and Markov (1916).

[19] In Letter 6 of 1896 to Chuprov, Bortkiewicz admitted that his exposition of the LSN was not quite satisfactory (in what respect?) but that he will possibly publish some corrections, see translation in Sheynin (1996, pp. 41 – 42). He did return to his LSN, but failed to correct it. I have also included Bortkiewicz' remarks to the effect that Markov did not really understand anything in statistics except its purely mathematical substance (Sheynin 1996, pp. 42 – 43).

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